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WILLIAM NATHAN SCHOENFELD (1915–1996): INNOVATIVE SCIENTIST, INSPIRING TEACHER, RELENTLESS QUESTIONER, COMPLICATED MAN

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The scientific and academic contributions of the late William N. Schoenfeld (1915–1996) are large and, together with his personal qualities as a researcher, thinker, and teacher, supply the themes for this memorial essay on his work and life.

Key words: behaviorism, learning theory, secondary reinforcement, avoidance behavior, experimental anxiety, schedules of reinforcement, variability of response, religious behavior

> While I am a psychologist by profession, I am a student of other things by interest. I do not think I am a dilettante, but rather simply a student who likes to study things and would, if I could, study everything. (Schoenfeld, 1993, p. xviii)

Fred Keller and Nat Schoenfeld were men whose names are strongly linked in the history of the scientific movement that eventually led to the creation of this journal. Together at Columbia College in 1946, they originated the pioneering introductory psychology course in which students learned about fundamental principles of behavior mainly through actual laboratory experimentation rather than through standard methods involving textbooks, lectures, and classroom demonstrations. Keller and Schoenfeld's classic text *Principles of Psychology,* published a few years later (1950), drew heavily on Skinner's 1938 volume *The Behavior of Organisms* for its overall themes, but was also an attempt to integrate the basic phenomena of so-called reinforcement theory with important work done by many other investigators of learning and conditioning. Furthermore, it tried to relate this body of knowledge to topics of historic significance in various subject-matter areas of general psychology: for example, psychophysical methods, Donders' reaction-time techniques, activity rhythms, Freudian repression and regression, theories of conflict behavior, the astronomer's personal equation, the context theory of meaning, imitation, cooperation, and personality consistency. Unlike Skinner's volume, *Principles of Psychology* offered practical applications whenever they seemed reasonable. There is little doubt that K & S (the nickname by which the book is usually known; it was reprinted in 1995), plus the laboratory courses that arose elsewhere based on features of the Columbia model, were more influential in sparking a growth of interest in the Skinnerian approach than was *The Behavior of Organisms* itself.

These facts are well known to most readers of this journal, partly because of the many recent eulogies and tributes that Keller received after his death on February 2, 1996. It is a curious coincidence that the other half of the Keller and Schoenfeld team passed away only 6 months later and that both died during the year that marks the 50th anniversary of the establishment of Columbia's program.

Nat Schoenfeld's impact on the field of learning and conditioning was large, and he was the mentor of many students who made

I thank the many colleagues and former students of Nat Schoenfeld who wrote or transmitted to me judgments and reminiscences about him—descriptions of the impact of his teaching and research, the ways in which he behaved as a scientific collaborator, the opinions he strongly held about many topics (both inside and outside psychology), and the personal characteristics he possessed that could inspire, challenge, surprise, and even intimidate or infuriate them. Without specific attributions, I have tried to blend these sketches and comments together into an integrated picture of the man, which was not so easy to do in a short article about a person who many correspondents described as one of the most complex individuals they had ever known. However, there was more agreement among the various correspondents than I had expected.

Reprints of this article may be obtained from Eliot Hearst, Department of Psychology, Schermerhorn Hall, Columbia University, New York, New York 10027. A complete curriculum vitae is currently being assembled for Schoenfeld, which will be available to readers who desire more numerous and specific citations than are supplied here.

significant contributions to basic and applied fields of behavioral psychology and served on the editorial boards of *JEAB* and *JABA*. His writings and research were extremely diverse in their scope and often innovative in their conceptualization. But perhaps they are relatively unfamiliar to the younger scientists among this journal's readers—compared to Keller's work on personalized instruction, for example. However, Keller (1977, p. 76) himself stated that the Columbia program, ''for which I usually get the credit owed more to him than it did to me,'' and that most of its success ''must be attributed to him'' (Keller, 1982, p. 84). He also noted in the 1977 passage that Nat ''supplied the skills and knowledge that I lacked, as he had done in our Morse-code collaborations.'' As an undergraduate and graduate student at Columbia in the early 1950s and a long-time observer of the Columbia scene since then, I do not think that Keller was just being his usual modest self when he made such statements.

After having taken the now-classic Psychology 1–2 introductory lab course in 1951–1952 and being inspired to concentrate on psychology by the experiments we performed and Keller's rather easygoing but charming and informative lectures, I first encountered Nat Schoenfeld in a 1-year sequence of undergraduate courses covering general experimental psychology, the first titled Discrimination and the second Motivation. Nat's style was remarkably different from Fred's—openly skeptical of almost any idea, often confrontational, inevitably challenging. There were only six students in the class, and five of them went on to earn PhDs and become active workers and teachers in various fields of psychology. With 2 scheduled hours of morning lectures and two 3-hr afternoon lab sessions or discussions per week, the courses warranted the 5 credit hours assigned to them each semester.

It did not take long for students to realize that Schoenfeld's courses were not going to be taught in a traditional way by a traditional teacher. Nat would talk about almost anything and managed to elicit high-spirited discussions not only on problems in psychology but also on virtually any topic that happened to catch his fancy that particular day: literature, politics, religion, and so forth. Sometimes I thought his undergraduate classes were as much a continuation of Columbia's renowned Humanities and Contemporary Civilization core program as they were basic psychology courses. I do not think that most of the class time was devoted to required topics that both the syllabus and course catalogue description enumerated. Nat's goal was to teach students to be analytical, critical, and careful—regardless of the subject matter under discussion. His vast knowledge of the sciences and humanities was impressive, and he was the best teacher I ever had. I will describe the personal ways in which he affected students and colleagues, and allude to some of their specific reminiscences and remarks about him, after a look at the major (and some minor!) contributions he made as a researcher and thinker.

Schoenfeld as a Researcher and Theorist

Even those who knew Nat well may be surprised to learn that his 1942 PhD was obtained in social psychology and sponsored by Otto Klineberg. The thesis examined social stereotypes and asked whether there might be a ''kernel of truth'' that provided some factual basis for nationality and name stereotypes. (Is Cuthbert a ''sissy,'' Agatha ''middle-aged,'' Austin ''cultured''?) His data indicated a high degree of consistency among subjects' ratings, even for name stereotyping, and he argued that there could be no factual basis for this outcome, thus weakening the case for the kernel-of-truth hypothesis (of course it was possible, for example, that Agatha was an uncommon name to give babies in the early part of the 20th century and women with that first name were likely to have been born before 1900).

At about this time Nat also published research on relationships between ego-involvement and confidence judgments (legend has it that he invented the word *ego-involvement* but I doubt whether he was the first to use it), and on the Gestalt concept of closure. He also became friendly with Fred Keller and collaborated in some of Fred's work on Morse Code learning, as well as in experiments with another colleague on psychophysical judgments involving pitch discriminations. But, after Keller introduced him to Skinner's book, Nat's interests gravitated more and more toward research and theory in learning and conditioning. He and his graduate students performed work on unconditioned bar-press

William Nathan Schoenfeld
December 6, 1915–August 3, 1996

rates (operant levels) of rats in a Skinner box and, more importantly, on the training conditions necessary for the establishment of a secondary reinforcer (out of which arose his influential and oft-tested hypothesis that, to acquire reinforcing powers, a neutral stimulus must first come to serve as a discriminative stimulus for some response: see Schoenfeld, Antonitis, & Bersh, 1950).

In 1952 he and Phil Bersh wrote a penetrating review of Clark Hull's final book *Essentials of Behavior,* especially criticizing its preoccupation with dubious quantitative methods and its neglect of trial-spacing and partial-reinforcement effects (two topics that continue to interest students of learning and behavior today). Two years later the very important multiauthored book *Modern Learning Theory* appeared, which subjected the leading theorists of the day to analytic (and sometimes devastating) criticism. A chapter by Nat and coauthor Conrad Mueller was a concise, fair, but ultimately negative evaluation of Edwin Guthrie's approach to learning.

In typical Schoenfeld fashion—trying to define, evaluate, and integrate familiar psychological concepts in an objective, operational way—he wrote a highly influential chapter (Schoenfeld, 1950a) that sought to systematically relate the effects of so-called anxiety, escape, avoidance, and punishment procedures within an approach that was similar to Mowrer's but posited no internal drive states like ''fear.'' Instead, for example, Schoenfeld argued that an avoidance response emerges mainly because (besides external cues) the proprioceptive-kinesthetic stimuli associated with all other responses of the subject are followed by an aversive event (a main inspiration for Murray Sidman to develop the avoidance procedure that bears his name). Nat ridiculed the idea that nonoccurrence of an event could be a reinforcer for avoidance responding, a point that never seemed so convincing to me because if predictable, expected events do not occur, subjects ought to discriminate their absence and be affected by their omission (some readers may know that I have been interested for a long time in the behavioral effects of stimulus nonoccurrence, deletion, and absence—the role of ''nothing'' in psychology and other fields).

In later years Nat expressed unhappiness with his theoretical approach to avoidance and punishment because it was virtually impossible to measure these presumptive proprioceptive cues and because ''all other responses of the subject'' constituted an infinitely large class of behaviors. And, following similar logic, I don't believe he was very favorable toward my use of an ''internal cues hypothesis'' to explain why generalization gradients, obtained by varying aspects of the external environment after Sidman avoidance training, were extremely flat. Towards the end of his career he was very critical of contemporary behaviorists' usage (following Skinner) of ''private events'' to interpret various phenomena, because he felt that they were not much different from talking about ''cognitions'': vague and impossible to measure. He came to think that today's behaviorism was not behavioristic enough!

Schoenfeld's early work on heart-rate conditioning in humans (primarily in collaboration with Phil Bersh and Joe Notterman) reflected these interests in experimental anxiety, a topic that came to occupy much of his time over the next 20 years and was eventually extended to rats and monkeys. There was considerable excitement in the 1950s about his group's discovery that signals for shock produced a heart-rate deceleration whereas the unconditioned stimulus (US) itself led to an acceleration (see Notterman, Schoenfeld, & Bersh, 1952). This outcome questioned traditional beliefs about Pavlovian conditioning that posited similarity if not identity between conditioned responses (CRs) and unconditioned responses (URs). Foreshadowing Nat's later deep involvement with and questioning of the operant-respondent distinction, he and his colleagues also suggested the possibility of skeletal mediation as a factor: Subjects might hold their breath during the pre-US period, thus indirectly affecting heart rate. Now we know that heart-rate decelerations to a CS may be just the first part of a biphasic response, the remainder (acceleration) of which is detected only if measures are taken later in the CS period or to longer CSs.

When I became a graduate student of Nat's in 1953, Bill Cumming and I collaborated with him on constructing a framework (see Schoenfeld, Cumming, & Hearst, 1956) that would presumably encompass both interval

and ratio (e.g., break and run) behavior, even though the approach involved experimental manipulation of temporal factors only; the independent variable was not based on a dependent variable like number of responses. Despite that type of restriction, appropriate selection of values of the temporal independent variables produced behavior that resembled ratio behavior in many details. I remember my own excitement at realizing that various specific types of ratio and interval schedules, extinction, and so forth, seemed to fit neatly into a table that plotted cycle length against the proportion of the cycle in which a food reinforcement could occur. Ferster and Skinner (1957) used the label *limited hold* to refer to some similar schedules.

This entire framework was extended over the years to include progressively more complex schedules of signals and reinforcers, appetitive and aversive, and resulted in a 1972 book that Nat coauthored mainly with Brett Cole on *Stimulus Schedules.* Many interesting and very reliable effects of schedules emerged from the overall systematic formulation, which would not be considered theoretical by many scientists because no hypothetical constructs or intervening variables were posited; the experimenter simply studied the interactive effects of objective, empirical variables. Contemporary researchers working on topics involving reinforcement schedules concentrate heavily on their data's relevance to the matching law, but they might find very provocative the work done within the framework of Schoenfeld's group even though, so far as I can tell, the matching law is not mentioned in the 1972 volume.

Schoenfeld also edited a book on *The Theory of Reinforcement Schedules* in 1970, which consisted of chapters presenting a variety of views on the topic, including one of my favorite articles, Herb Jenkins' analysis of sequential organization in schedules of reinforcement. Nat's idea of the ''behavior stream'' was stressed in his own chapter with John Farmer; simply put, the idea was that organisms are always doing something, and the observation and measurement of many other responses, as well as topographical changes in a supposedly specific target response, are being neglected in most analyses, where the focus is on individual responses and not their variety, sequencing, and the behavioral context in which they occur.

Perhaps Schoenfeld's most innovative suggestions concerned response variability. He was dismayed at the superficial ways in which *stimulus* and *response* were normally defined (their presumed ''generic nature'' did not impress him) and he argued that we ought to devote more time to studying (a) how various topographies of, say, a bar-press response changed depending on experimental conditions, and (b) whether response variability itself can be conditioned by rewarding subjects for varying the ways in which they performed a response or sequence of responses. He pointed out the relation of the latter topic to novelty and creativity in humans. A number of his students performed research along these lines, and over the years the topic has assumed increasing importance, as recent articles in journals of animal learning and behavior attest (see, e.g., Machado, 1994). A brief note of Schoenfeld's (1950b; republished in 1968) offered the provocative notion that resistance to extinction after partial reinforcement of an operant response is greater than after continuous reinforcement because many more subcategories of response have received reinforcement in the former case and therefore have to be extinguished; in the latter case initial conditioning presumably produces relatively stereotyped behavior. Today researchers remain interested in Nat's clever suggestion, which to my knowledge has never been convincingly tested or disproved.

Wherever possible, Schoenfeld favored the use of a parametric analysis so that the experimenter could see the effects of some variable over a wide range of its possible values, as opposed to choosing what seemed to be some optimal value and conducting research with just that value. Besides allowing an experimenter to better judge the generality of a particular result, a parametric analysis also encourages attempts to derive mathematical equations based on an organism's differential responding to a variety of stimulus values.

Because Nat had so many interests, some of the topics he wrote about or spoke about do not fit easily into the picture I have presented above. His 1973 EPA Presidential Address discussed the topic of race differences in intelligence as a bit of ''psychological non-

sense.'' While still at Columbia he participated (with W. W. Cumming) in an extensive project that examined verbal dependencies in language behavior and in a smaller project (with W. Lener) involving *Oncopeltus fasciatus* that investigated mating behavior in that organism as well as how to ascertain its sex (recalling the young Freud's initial research to determine the sex of an eel; how many readers know what kind of organism Lener and Schoenfeld were studying?). Late in his career he began research on mathematical behavior in young children and on what ''counting'' involves (certainly a major topic today in human cognitive psychology, but not neglected in contemporary animal research, either; see Boysen & Capaldi, 1993); he spoke on this topic when he delivered his Division 25 Presidential Address in 1975. During his career he wrote chapters or notes relating learning theory to social psychology, perception, pain and suffering, and humanism. A pleasant finale to his career was overseeing the actual publication of *Religion and Human Behavior,* a book he had written in a 4-month burst of energy in 1971, but which was not available to the general public until 1993.

A final note about Schoenfeld's contributions. When *JEAB* began publication in 1958, Nat worked closely with his second wife, Serena, an expert in journal production and commercial printing, to design the journal's cover (its colors, dimensions, layout, and calligraphy). *JEAB* may be one of the few, if not the only, psychology journal that has never changed its cover design in all the years of its life! In an interesting reminiscence (Schoenfeld, 1987) he described the details of this project and in addition mentioned his uneasiness with choice of the phrase *experimental analysis,* which he thought involved a ''touch of pretension and proclamation.''

Schoenfeld as a Teacher and Person

As I suggested in the introduction to this commemorative essay, Nat Schoenfeld was a challenging taskmaster. The imposing reading lists he gave students at the beginning of graduate courses (some ranging, I think, as long as 50 to 60 pages), or the extensive reading material supplied to undergraduate students on the procedure sheets for planned class experiments, were enough to scare even the most diligent pupils. Only later did students discover that he never checked up on whether they had done most of the readings; but, from the debates that raged in the classroom, he could usually tell which students had acquired some understanding of the basic concepts and which had not.

A person who was assigned to lead the discussion of a particular topic in a graduate seminar had better be prepared. Nat was a relentless questioner and would rarely let more than 5 minutes pass without interrupting a speaker with remarks like ''What do you mean by that?''; ''What's the evidence for that?''; or ''So what?'' And the discussions might shift fairly suddenly to topics, inside and outside psychology, that one might never have predicted to arise. At Queens College in the 1970s, no matter what the course title, graduate students called his courses ''Advanced Schoenfeld.'' Perhaps the biggest verbal reinforcement he would ordinarily give was, ''That's kind of interesting.''

Nat's goal was not mainly to instill facts but to teach students how to pursue intellectual truth, to train them to analyze and criticize, to transform them into skeptics regardless of what the subject matter under discussion might be. He was probably much noisier than Socrates. All students admired and respected his brilliant intellect and vast knowledge. (He was the only psychology major at CCNY during his years as an undergraduate there to obtain a BS rather than a BA in psychology, because he had taken nonrequired courses in physics, chemistry, mathematics, geology, and biology—simply, he said, following his interests. And later on he was successful in adding ''some background in other sciences'' as a desirable factor in selection of incoming graduate students at Columbia.) However, as readers will have guessed, some students found his approach to teaching discomforting and intimidating; they might end up being totally silent, even tearful, and at times angry. Others enjoyed the sessions tremendously, even though I cannot recall a single instance when Nat could be said to have lost an argument. He would often say to students and visiting outside speakers, ''Nothing personal, you understand,'' while interrogating them mercilessly.

Schoenfeld was very demanding in his expectations for graduate students. If you were a hard worker, he was likely to tolerate your

mistakes and overlook some of your foolish comments. If you didn't know much about laboratory equipment and programming circuitry, he might make you build a lab from scratch. Laziness, procrastination (he replied almost instantaneously to letters and messages he received), and lack of punctuality were qualities that were almost certain to cause him to place you into either his ''negative'' or ''questionable'' categories of students. (However, later in his life he often ruminated about how difficult it was to predict whether a bright and productive graduate student would have a successful career.) Sometimes in his graduate courses he wrote the names of latecomers on the blackboard, and occasionally he threatened to lock the classroom door after the scheduled starting time. When classes fell behind the course's scheduled plan—not uncommon because of the usually fascinating digressions he could not resist pursuing—he would often suggest that students meet at 6:00 a.m. on a forthcoming day to make up the missing work. Classes that called his bluff found him there at the appointed hour (he was a very early riser, anyway) but he rarely asked them to do that again. On one occasion he insisted that his graduate students come in for a meeting at 8:00 a.m. on New Year's Day; everyone complied, although no one had slept the night before.

Against this background, it may be surprising how many students and colleagues described him as kind, generous, sensitive, mischievous, and exceptionally devoted and loyal to his students and to staff members he hired to work for him. He confessed to crying at Italian operas. Those who were suffering problems in their love lives found him unexpectedly perceptive about their condition, and he would go out of his way to be attentive and reinforcing. Rather than pressuring a student into accepting some prestigious job offer that he would have preferred the individual to accept, he would help the person choose the position that he or she was likely to enjoy the most. He was constantly available to chat with students, and many of them recall discussions in his office or the animal vivarium when he listened with initial patience to new experimental ideas or merely expressed, sometimes in a monologue, his own beliefs about psychology and life. Colleagues and students soon realized that he had fairly conservative political views—not too widely held at universities in New York City then or now—but he could (usually) transform private conversations about such topics into relatively objective interchanges.

With justification, many students and coworkers felt that Schoenfeld was particularly demanding, even unfair, in his treatment of female students. He sponsored relatively few of them, and one female graduate student recently wrote me that he had not been encouraging in her quest for a PhD (''no one will date you if you have a PhD''; ''you'll just get married and never contribute to the field''), and he once openly admitted to her that he was a male chauvinist (which she pointed out might be related to his political and religious views; as a practicing Jew, whose strict observance of the rules and restrictions of his religion increased as he grew older, he held orthodox beliefs about the place of women in society). On the other hand, some correspondents of mine said that his views on many subjects mellowed after he left Columbia for Queens College in 1966, and one of his female PhD students from Queens called him a dear friend whose wisdom, integrity, and incomparable ability to go right to the heart of things were very important aspects of the supportive environment he supplied her with. She expressed the opinion, echoed by many others, that he was just not aware of the power of intimidation he exerted over people. ''What did I do?'' he might ask a colleague when it was obvious he had greatly upset someone.

Those who attended the 1989 ABA meetings in Milwaukee, which included sessions on the Columbia program of the 1950s and related topics (partially in honor of Fred Keller's 90th birthday), may recall Schoenfeld's extemporaneous talks there. As usual he did not speak much about topics pertinent to the titles of the presentations he was assigned, but, in reminiscing about his career and life, he was funnier than many stand-up comics we had listened to on television or in nightclubs (a skill with jokes and stories I had never seen him exhibit in the past). Because he had been relatively immobile for years due to a serious automobile accident, at that ABA meeting his former students (myself included) took turns walking to nearby markets to

buy food for him that did not violate Jewish dietary rules.

It so happened that four or five master or near-master chess players received doctoral degrees with Nat. In Schermerhorn Hall at Columbia in the 1950s there were several chessboards always available, and games were often in progress among students and faculty of all playing strengths, which some other, chess-negative professors and students thought was inappropriate for that setting. At any rate, somehow I feel that Nat was unduly respectful of and easy on us chess experts, compared to other students (that is one reason why I obtained opinions from various coworkers and students of his before I started composing this essay of appreciation, although I was certainly well aware that he could be moody and that many who knew him would make as many negative as positive comments about him). Nat was not a very good chess player. He spent so much time trying to grasp underlying general strategic concepts that he would often overlook obvious threats or make glaring blunders of his own.

The last time I saw Nat was during the Thanksgiving break of 1995 at his retirement residence in Sun City West, Arizona. He was obviously very ill, taking oxygen through a nasal attachment and primarily confined to an adjustable chair-bed, although he did use a walker to come to the dining room for the lunch his wife Melanie served us. This time I avoided any debates, just listened and asked a few questions as he ruminated on life, religion, psychology, and people. First he talked for a while about Jesus's birth and wondered aloud why so little was known about details of his early life. (Did Mary ever tell him he was ''illegitimate''? When and how did he realize that he was ''special''?) Discussing pain and suffering, Nat criticized doctors for worrying about providing him with too much morphine; because his illness was probably terminal, there seemed no point in fretting about the possibility of an addiction (his close friends knew that he had conquered an addiction to morphine as a young man of 21, a dependency initiated by its use in relieving his pain after an operation).

He acknowledged that in recent years his goals had shifted from understanding behavior to contemplating the question of ''what is right?'' The most important thing in life, he continued, was personal relationships. Now his happiest moments involved phone conversations and visits from his children.

I tried to elicit some specific memories about famous psychologists he had known or worked with. ''Met J. B. Watson once for a few minutes and was too intimidated to talk much with him,'' he recalled. The meeting was in connection with some marketing research on deodorants that Nat and another graduate student at Columbia were working on, with funding from Watson's advertising firm. In this project attractive young women rode stationary bicycles until they perspired profusely. Then raters judged the odor-reducing effectiveness of different commercial products.

''I remember my meeting with James McKeen Cattell, too, in 1941 at the 50th anniversary celebration of Columbia's department, which Cattell had originally founded. I was in charge of handling the cloakroom at the banquet and later Cattell, then in his 80s, offered to give me advice on how to become a successful psychologist.'' Nat was not too impressed with Cattell's two-word counsel: ''WORK HARD!'' Nat said that he had really admired R. S. Woodworth and A. T. Poffenberger, long-time members of Columbia's department. ''They were true gentleman-scholars: not too many of those around today.''

''I was never a follower or a true believer in anything very specific in psychology,'' he continued, ''but I am certainly a behaviorist. I don't think behaviorism is a philosophy, as many say, but merely a statement of what behavior science is about. Cognitive psychologists are too mentalistic and even today's behaviorists are developing mystical tendencies (including Skinner himself in his final years). Skinner was not really self-critical enough and he always felt uncomfortable with me because he could tell I didn't swallow everything he said.''

When I was ready to leave, he seemed almost as eager to talk as when I had arrived 3 or 4 hours before. ''I really think I had more effect as a teacher than with anything else I did. But popularity itself was never my goal; I wanted to stimulate thought and criticism, more in posing and dissecting questions than in answering them.''

Nat Schoenfeld: sometimes humble, sometimes arrogant; often kind and often confron-

tational; frequently open-minded and frequently dogmatic. Always asking questions. A complicated and very special man.

Finale

William Nathan Schoenfeld was born December 6, 1915, in New York City and died August 3, 1996, in Sun City West, Arizona, after a long illness. He received a BS from the City College of New York in 1937, an AM from Columbia in 1939, and a PhD from Columbia in 1942. All his degrees were in the field of psychology. He advanced from a lecturer to a full professor (1958) at Columbia, which he left in 1966 for a position at Queens College of the City University of New York, where he was chair for 2 years and retired as a professor emeritus in 1983. Having served as a visiting professor at Bar-Ilan University in Israel from 1980 to 1981, he assumed the same title at the Hebrew University of Jerusalem from 1983 to 1993. Besides being a fellow of many psychological societies and associations and on the board of editors of many journals, Schoenfeld was President of APA's Division 25 from 1973 to 1976, President of the Eastern Psychological Association from 1972 to 1973, and President of the Pavlovian Society of North America from 1971 to 1972. He served as chair of the NIMH Study Section in Experimental Psychology from the late 1960s to early 1970s and held various appointments at universities in Brazil, Mexico, and Venezuela. Near the end of his career he was pleased to receive an honorary doctorate from the University of Guadalajara, Mexico (1993). His survivors include his wife Melanie and their three children, Rivka, Joshua, and Naomi, as well as a son by a previous marriage, Mark.

Columbia University's Psychology Department is opening a new state-of-the-art ''electronic classroom-laboratory'' in 1997, where students will be able to take a science-credit course that is very similar to the one Keller and Schoenfeld initiated in 1946 at Columbia. Undergraduates will work with individual rats or pigeons for much of the course, but will also conduct experiments on human memory, perception, and psychophysics. This new suite of rooms will be called the Keller and Schoenfeld Undergraduate Laboratory, honoring the devotion to undergraduate education of these two great but very different kinds of teachers.

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